

CORRESPONDENCE

To the Editors:

I am quite pleased by Carlos Alvarado's review of my book *Dizionario del Paranormale* and I would undoubtedly agree with him on many of the points raised IF mine was an academic or scientific work as he intends it to be in his review. However, the book was aimed at the general public and could not have been a complete treatise on Parapsychology. I am quite aware of the limitations and omissions in the book but, as I stated in the Introduction to the *Dizionario*, my specific aim was simply to provide readers with some balance to similar works previously published in Italy which totally ignored the skeptical viewpoint.

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To the Editors:

I have pursued ESP research off and on for sixty years, and have obtained consistently negative results, with the possible exception of some recent borderline data using the Schmidt Psi Tester.¹ For my Master's thesis (Crumbaugh, 1938) I did more ESP trials than Rhine had done when he published his first book (Rhine, 1935). I had two grants from the Parapsychology Foundation of New York to spend the summer of 1954 at the Duke Laboratory learning the Rhine techniques, and then to return to Memphis

¹ The Schmidt Psi Tester is a RNG device using a computer chip. The 3LMP "game" presents a PK experiment (which could also be precognitive ESP) to elicit the subject's attempt to cause a colored jeweled lamp to light upon pressing the corresponding colored pushbutton. Three buttons with corresponding lamp colors offer one in three chances to succeed by pure chance. A high school female subject did 4,500 trials with 1,572 hits, 72 above chance expectation. The standard deviation was 31.61 with a critical ratio of 2.28.

State (now University of Memphis) where I then taught psychology and to continue my ESP research there. The results of my studies continued to be negative. Palmer (1998) has suggested that the main value of my work may be the fact that psi did not occur significantly in such a large body of data whereas the laws of chance would predict that at least a few segments of such data would indicate a spurious significance by pure chance.

This suggests that a significant suppressor mechanism may have been at work. Ordinarily one would expect an experimenter with so much negative data to lose interest and discontinue work. But, because members of my family and close friends have reported very impressive "anecdotal" data, my interest has continued while I maintain—as I have over my career as a psychologist—that psi can be scientifically proved only by a truly repeatable experiment, in the classical sense of a design in which the variable studied can be varied, isolated, and repeated with consistent results.

The obvious suggestion here is that something about the experimenters, subjects, or both must be different, for some, all of the time, and, for others, at unknown times, which suppresses positive responses. While Schmeidler (1945) has found evidence that results are influenced by the attitudes and other personality traits of subjects and experimenters, there is no study selecting both subjects and experimenters by a thorough battery of both clinical and psychometric types with a view toward analyzing the interactional effects of subjects and experimenters of various personality patterns.

I believe that some sort of brain field independent of space and time may create the conditions necessary for the operation of psi effects. An apparently paradoxical effect may pertain in the case of negative results. This is not new, and physicists say it is not a reasonable assumption, but science has been often revised by new data. I don't have the data, but field theory and quantum mechanics offer much room for speculation—something not offered by precious *Journal* space here.

So I will say only that as a physical monist (which Rhine was also, though nobody seemed to believe him) I look to a physical mechanism which seems to violate the inverse square law. The a posteriori data of parapsychology suggest it is there. And the experience of Pasteur a hundred years ago shows the fallacy of rejecting a posteriori data on purely a priori grounds. The late, great neuropsychologist, D. O. Hebb (1951), also fell into this trap in evaluating psi effects.

In the cases of clairvoyance and PK there may be no secondary human (or animal) fields, but there may be patterns held within objects themselves which vary with time and thus become differentially sensitive to human attempts to contact them. The human sender or receiver may have patterns of variation of the fields which create some extrasensory

interaction that makes objects responsive to them.

The correct conclusion should be that neither skeptics nor supporters of a given hypothesis (such as the basic psi hypothesis) are necessarily wrong, and that each may be right under certain conditions. The key, of course, is gaining an understanding of these conditions. Skeptics have on their side the fact that the postulated phenomena are very ephemeral, vague, subtle, unstable, and weak in most manifestations. Believers have on their side the support of extensive experimental results. But skeptics challenge these studies, and emphasize the need for a truly repeatable experiment in the classical sense. The standoff can be resolved only when the weak, subtle and unstable conditions can be analyzed and controlled to the extent that a fixed experimental and fully repeatable design can be offered.

Rhine (1959) and Schmeidler (1945) have suggested that at least a part of such subtle effects lies in the personalities of experimenters and their subjects. It is admittedly a difficult area of experimentation, because most experimenters and subjects who have had consistently negative results lose interest, and it is hard to remotivate them for further study. They are not challenged by the hope of determining *why* they failed; they are likely to have concluded that if they can't do it, nobody can, and that all positive results are in some way bad scientific methodology or bad security against sensory cues and/or other experimental errors. The importance of this experimental area is, however, in my opinion, well worth the effort. The lack of motivation probably can, in a sufficient number of cases, be regenerated. (Unfortunately, I cannot accept this challenge myself because, in retirement, I no longer have the necessary experimental facilities, and I also have some ongoing commitments which still are unfinished.)

One final observation: As I finished the last 30 runs of my own work on the Psi Tester's Game 3LMP (Schmidt, 1996-1997) I obtained 320 hits or twenty trials above chance expectation out of the 900 trials, with a CR of 1.5. While this is obviously insignificant, it could become significant if the same scoring rate could be maintained over time. But I noticed something different about this batch of data: It was gathered in a period of 90 minutes while I was somewhat angry with a service person who had broken an appointment without notice and instead had come at an inconvenient time.

Is it possible that normally chance-scoring subjects and experimenters can score significantly and positively only within a period of arousal by a negative emotion? ESP studies have generally indicated that a positive emotional state yields best results. Could it be that at least some experimenters and subjects can become positive only under negative conditions? Perhaps there are certain personality patterns for which this is true.

The study of reasons for negative psi results may be as important as positive findings in gaining acceptance of parapsychology by the "orthodox" or main body of science.

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